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# EXPERIMENTS

UPON

# MAGNESIA ALBA, QUICK-LIME,

AND OTHER

# ALCALINE SUBSTANCES;

By JOSEPH BLACK, M. D.
Profeſſor of CHYMISTRY in the Univerſity of Edinburgh.

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To which is annexed,

## An ESSAY on the COLD
## Produced by EVAPORATING FLUIDS,

AND

## Of ſome other means of producing COLD;

By WILLIAM CULLEN, M. D.
Profeſſor of MEDICINE in the Univerſity of Edinburgh.

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# EXPERIMENTS

# UPON

# MAGNESIA ALBA,

# QUICK-LIME,

# AND OTHER

# ALCALINE SUBSTANCES.

## PART I.

HOFFMAN, in one of his obſervations, gives the hiſtory of a powder called *Magneſia Alba*, which had been long uſed and eſteemed as a mild and taſteleſs purgative; but the method of preparing it was not generally known before he made it public †.

It was originally obtained from a liquor called the *mother of nitre*, which is produced in the following manner:

Salt-petre is ſeparated from the brine which firſt affords it, or from the water with which it is waſhed out of nitrous earths, by the proceſs commonly uſed in cryſtallizing ſalts. In this proceſs the brine is gradually diminiſhed, and at length reduced to a ſmall quantity of an unctuous bitter ſaline liquor, affording no more ſalt-petre by evaporation; but, if urged with a briſk fire, drying up into a confuſed maſs which attracts water ſtrongly, and becomes fluid again when expoſed to the open air.

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† Hoff. op. T. 4. p. 479.

To this liquor the workmen have given the name of the *mother of nitre*; and Hoffman, finding it compoſed of the *magneſia* united to an acid, obtained a ſeparation of theſe, either by expoſing the compound to a ſtrong fire in which the acid was diſſipated, and the *magneſia* remained behind, or by the addition of an alkali which attracted the acid to itſelf: And this laſt method he recommends as the beſt. He likewiſe makes an inquiry into the nature and virtues of the powder thus prepared; and obſerves, that it is an abſorbent earth which joins readily with all acids, and muſt neceſſarily deſtroy any acidity it meets in the ſtomach; but that its purgative power is uncertain, for ſometimes it has not the leaſt effect of that kind. As it is a mere inſipid earth, he rationally concludes it to be a purgative only when converted into a ſort of neutral ſalt by an acid in the ſtomach, and that its effect is therefore proportional to the quantity of this acid.

Although *magneſia* appears from this hiſtory of it to be a very innocent medicine; yet, having obſerved, that ſome hypochondriacs who uſed it frequently, were ſubject to flatulencies and ſpaſms, he ſeems to have ſuſpected it of ſome noxious quality. The circumſtances however which gave riſe to his ſuſpicion, may very poſſibly have proceeded from the imprudence of his patients, who, truſting too much to *magneſia*, (which is properly a palliative in that diſeaſe), and neglecting the aſſiſtance of other remedies, allowed their diſorder to increaſe upon them. It may indeed be alledged, that *magneſia*, as a purgative, is not the moſt eligible medicine for ſuch conſtitutions, as they agree beſt with thoſe that ſtrengthen, ſtimulate, and warm; which the ſaline purges commonly uſed are not obſerved to do. But there ſeems at leaſt to be no objection to its uſe, when children are troubled with an acid in their ſtomach; for gentle purging in this caſe is very proper, and it is often more conveniently procured by means of *magneſia* than of any other medicine, on account of its being intirely inſipid.

The above-mentioned author obſerving, ſome time after, that a bitter ſaline liquor, ſimilar to that obtained from the brine of ſalt-petre, was likewiſe produced by the evaporation of thoſe waters which contain common ſalt, had the curioſity to try if this would alſo yield a *magneſia*. The experiment ſucceeded: And he thus found out another proceſs for obtaining this powder, and at the ſame time aſſured himſelf by experiments, that the product from both was exactly the ſame *.

My curioſity led me ſome time ago to inquire more particularly into the nature of *magneſia*, and eſpecially to compare its properties with thoſe of the other abſorbent earths, of which there plainly appeared to me to be very different kinds, altho' commonly confounded together under one name. I was indeed led to this examination of the abſorbent earths, partly by the hope of diſcovering a new ſort of lime and lime-water, which might poſſibly be a more powerful ſolvent of the ſtone than that commonly uſed; but was diſappointed in my expectations.

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* Hoff. Op. T. 4. p. 500.

I HAVE had no opportunity of ſeeing Hoffman's firſt *magneſia*, or the liquor from which it is prepared, and have therefore been obliged to make my experiments upon the ſecond.

IN order to prepare it, I at firſt em

ployed the bitter ſaline liquor called *bittern*, which remains in the pans after the evaporation of ſea-water. But, as that liquor is not always eaſily procured, I afterwards made uſe of a ſalt called *Epſom ſalt*, which is ſeparated from the bittern by cryſtallization, and is evidently compoſed of *magneſia* and the vitriolic acid.

THERE is likewiſe a ſpurious kind of Glauber ſalt, which yields plenty of *magneſia*, and ſeems to be no other than Epſom ſalt of ſea-water reduced to cryſtals of a larger ſize. And common ſalt alſo affords a ſmall quantity of this powder; becauſe, being ſeparated from the bittern by one haſty cryſtallization only, it neceſſarily contains a portion of that liquor.

Those who would prepare a *magneſia* from Epſom ſalt, may uſe the following proceſs:

Dissolve equal quantities of Epſom ſalt, and of pearl aſhes ſeparately, in a ſufficient quantity of water; purify each ſolution from its dregs, and mix them accurately together by violent agitation. Then make them juſt to boil over a briſk fire.

Add now to the mixture, three or four times its quantity of hot water; after a little agitation, allow the *magneſia* to ſettle to the bottom, and decant off as much of the water as poſſible. Pour on the ſame quantity of cold water; and, after ſettling, decant it off in the ſame manner. Repeat this waſhing with the cold water ten or twelve times, or even oftner, if the *magneſia* be required perfectly pure for chemical experiments.

When it is ſufficiently waſhed, the water may be ſtrained and ſqueezed from it in a linen cloth; for very little of the *magneſia* paſſes through.

The alkali in the mixture uniting with the acid, ſeparates it from the *magneſia;* which, not being of itſelf ſoluble in water, muſt conſequently appear immediately under a ſolid form. But the powder which thus appears, is not intirely *magneſia;* part of it is the neutral ſalt formed from the union of the acid and alkali. This neutral ſalt is found, upon examination, to agree in all reſpects with vitriolated tartar, and requires a large quantity of hot water to diſſolve it. As much of it is therefore diſſolved as the water can take up, the reſt is diſperſed through the mixture in the form of a powder. Hence the neceſſity of waſhing the *magneſia* with ſo much trouble; for the firſt affuſion of hot water is intended to diſſolve the whole of the ſalt, and the ſubſequent additions of cold water to waſh away this ſolution.

The caution given of boiling the mixture is not unneceſſary; if it be neglected, the whole of the *magneſia* is not accurately ſeparated at once; and, by allowing it to reſt for ſome time, that powder concretes into minute grains, which, when viewed with the microſcope, appear to be aſſemblages of needles diverging from a point. This happens more eſpecially when the ſolution of the Epſom ſalt and of the alkali are diluted with too much water before they are mixed together. Thus, if a dram of Epſom ſalt and of ſalt of tartar be diſſolved each in four ounces of water, and be mixed, and then allowed to reſt three or four days, the whole of the *magneſia* will be formed into theſe grains. Or, if we filtrate the mixture ſoon after it is made, and heat the clear liquor which paſſes through, it will become turbid, and depoſite a *magneſia*.

I HAD the curioſity to ſatisfy myſelf of the purgative power of *magneſia*, and of Hoffman's opinion concerning it, by the following eaſy experiment. I made a neutral ſalt of *magneſia* and diſtilled vinegar; chuſing this acid as being, like that in weak ſtomachs, the product of fermentation. Six drams of this I diſſolved in water, and gave to a middle-aged man, deſiring him to take it by degrees. After having taken about a third, he deſiſted, and purged four times in an eaſy and gentle manner. A woman of a ſtrong conſtitution got the remainder as a briſk purgative, and it operated ten times without cauſing any uneaſineſs. The taſte of this ſalt is not diſagreeable, and it appears to be rather of the cooling than of the acrid kind.

HAVING thus given a ſhort ſketch of the hiſtory and medical virtues of *magneſia*, I now proceed to an account of its chemical properties. By my firſt experiments, I intended to learn what ſort of neutral ſalts might be obtained by joining it to each of the vulgar acids; and the reſult was as follows.

Magnesia is quickly diſſolved with violent effervefcence, or exploſion of air, by the acids of vitriol, nitre, and of common ſalt, and by diſtilled vinegar; the neutral ſaline liquors thence produced having each their peculiar properties.

That which is made with the vitriolic acid, may be condenſed into cryſtals ſimilar in all reſpects to Epſom ſalt.

That which is made with the nitrous, is of a yellow colour, and yields ſaline cryſtals, which retain their form in a very dry air, but melt in a moiſt one.

THAT which is produced by means of ſpirit of ſalt, yields no cryſtals; and, if evaporated to dryneſs, ſoon melts again when expoſed to the air.

THAT which is obtained from the union of diſtilled vinegar with *magneſia*, affords no cryſtals by evaporation, but is condenſed into a ſaline maſs, which, while warm, is extremely tough and viſcid, very much reſembling a ſtrong glue both in colour and conſiſtence, and becomes brittle when cold.

BY theſe experiments *magneſia* appears to be a ſubſtance very different from thoſe of the calcarious claſs; under which I would be underſtood to comprehend all thoſe that are converted into a perfect quick-lime in a ſtrong fire, ſuch as lime-ſtone, marble, chalk, thoſe ſpars and marles which effervеſce with aqua fortis, all animal ſhells, and the bodies called *lithophyta*. All of theſe, by being joined with acids, yield a ſet of compounds which are very different from thoſe we have juſt now deſcribed. Thus, if a ſmall quantity of any calcarious matter be reduced to a fine powder, and thrown into ſpirit of vitriol, it is attacked by this acid with a briſk effervеſcence; but little or no diſſolution enſues. It abſorbs the acid, and remains united with it in the form of a white powder, at the bottom of the veſſel, while the liquor has hardly any taſte, and ſhews only a very light cloud upon the addition of an alkali *.

THE ſame white powder is alſo formed when ſpirit of vitriol is added to a calcarious earth diſſolved in any other acid; the vitriolic expelling the other acid, and joining itſelf to the earth by a ſtronger attraction; and upon this account the *magneſia* of ſea-water ſeems to be different from either of thoſe deſcribed by *Hoffman*. He ſays expreſsly, that the ſolutions of each of his powders, or, what is equivalent, that the liquors from which they are obtained, formed a coagulum, and depoſited a white powder, when he added the vitriolic acid *; which experiment I have often tried with the marine bittern, but without ſucceſs. The coagulum thus formed in the mother of nitre may be owing to a quantity of quick-lime contained in it; for quick-lime is uſed in extracting the ſalt-petre from its matrix. But it is more difficult to account for the difference between Hoffman's bittern and ours, unleſs we will be ſatisfied to refer it to this, that he got his from the waters of ſalt ſprings, which may poſſibly be different from thoſe of the ſea.

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* Mr Margraaf has lately demonſtrated, by a ſet of curious and accurate experiments, that this powder is of the nature, and poſſeſſes the properties, of the gypſeous or ſelenitic ſubſtances. That ſuch ſubſtances can be reſolved into vitriolic acid and calcarious earth, and can be again compoſed by joining theſe two ingredients together. Mem de l'Acad. de Berlin. an. 1750, p. 144.

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* Hoff. op. T. 4. p. 48c & 500.

Magnesia is not leſs remarkably diſtinguiſhed from the calcarious earths, by joining it to the nitrous and vegetable acids, than to the vitriolic. Thoſe earths, when combined with ſpirit of nitre, cannot be reduced to a cryſtalline form, and, if they are diſſolved in diſtilled vinegar, the mixture ſpontaneouſly dries up in a friable ſalt.

Having thus found *magneſia* to differ from the common alkaline earths, the object of my next inquiry was its peculiar degree of attraction for acids, or what was the place due to it in Mr Geoffroy's table of elective attractions.

Three drams of *magneſia* in fine powder, an ounce of ſalt ammoniac, and ſix ounces of water, were mixed together, and digeſted ſix days in a retort joined to a receiver.

During the whole time, the neck of the retort was pointed a little upwards, and the moſt watery part of the vapour, which was condenſed there, fell back into its body. In the beginning of the experiment, a volatile ſalt was therefore collected in a dry form in the receiver, and afterwards diſſolved into ſpirit.

When all was cool, I found in the retort a ſaline liquor, ſome undiſſolved *magneſia*, and ſome ſalt ammoniac cryſtallized. The ſaline liquor was ſeparated from the other two, and then mixed with the alkaline ſpirit. A coagulum was immediately formed, and a *magneſia* precipitated from the mixture.

The *magneſia* which had remained in the retort, when well waſhed and dried, weighed two ſcruples and fifteen grains.

We learn by the latter part of this experiment, that the attraction of the volatile alkali for acids is ſtronger than that of *magneſia*, ſince it ſeparated this powder from the acid to which it was joined. But it alſo appears, that a gentle heat is capable of overcoming this ſuperiority of attraction, and of gradually elevating the alkali, while it leaves the leſs volatile acid with the *magneſia*.

Dissolve a dram of any calcarious ſubſtance in the acid of nitre or of common ſalt, taking care that the ſolution be rendered perfectly neutral, or that no ſuperfluous acid be added. Mix with this ſolution a dram of *magneſia* in fine powder, and digeſt it in the heat of boiling water about twenty-four hours; then dilute the mixture with double its quantity of water, and filtrate. The greateſt part of the earth now left in the filtre is calcarious, and the liquor which paſſed through, if mixed with a diſſolved alkali, yields a white powder, the largeſt portion of which is a true *magneſia*.

From this experiment it appears, that an acid quits a calcarious earth to join itſelf to *magneſia;* but the exchange being performed ſlowly, ſome of the *magneſia* is ſtill undiſſolved, and part of the calcarious earth remains yet joined to the acid.

When a ſmall quantity of *magneſia* is thrown into a ſolution of the corroſive ſublimate of mercury, it ſoon ſeparates part of the mercury in the form of a dark red powder, and is itſelf diſſolved.

Imagining that I perceived ſome reſemblance between the properties of *magneſia* and thoſe of alkalis, I was led to try what change this ſubſtance would ſuffer from the addition of quick-lime, which alters in ſuch a peculiar manner the alkaline ſalts.

Twenty-seven grains of *magneſia* in fine powder were mixed with eighteen ounces of lime-water in a flaſk, which was corked cloſe and ſhaken frequently for four days. During this time, I frequently dipp'd into it little bits of paper, which were coloured with the juice of violets; and theſe became green as ſoon as they touched the water, until the fourth day, when their colour did not ſeem to be altered. The water being now poured off, was intirely inſipid, and agreed in every chemical trial with pure water. The powder, after being perfectly well dried, weighed thirty-ſeven grains. It did not diſſolve intirely in ſpirit of vitriol; but, after a briſk effervеſcence, part of it ſubſided in the ſame manner as the calcarious earths, when mixed with this acid.

When I firſt tried this experiment, I was at the trouble of digeſting the mixture in the heat of boiling water, and did not then know that it would ſucceed in the heat of the air. But Dr Alſton, who has obliged the world with many curious and uſeful diſcoveries on the ſubject of quick-lime, having had occaſion to repeat it, I learned from him that heat is not neceſſary; and he has moreover added an uſeful purpoſe to which this property of *magneſia* may be applied; I mean the ſweetening of water at ſea, with which lime may have been mixed to prevent its putrefaction.

THAT part of the dried powder which does not diſſolve in ſpirit of vitriol, conſiſts of the lime ſeparated from the water.

QUICK-LIME itſelf is alſo rendered mild by *magneſia*, if theſe two are well rubbed together, and infuſed with a ſmall quantity of water.

By the following experiments, I propoſed to know whether this ſubſtance could be reduced to a quick-lime.

An ounce of *magneſia* was expoſed in a crucible for about an hour to ſuch a heat as is ſufficient to melt copper. When taken out, it weighed three drams and one ſcruple, or had loſt $\frac{7}{12}$ of its former weight.

I repeated with the *magneſia* prepared in this manner, moſt of thoſe experiments I had already made upon it before calcination, and the reſult was as follows.

It diſſolves in all the acids, and with theſe compoſes ſalts exactly ſimilar to thoſe deſcribed in the firſt ſet of experiments: But what is particularly to be remarked, it is diſſolved without any the leaſt degree of effervelſcence.

It ſlowly precipitates the corroſive ſublimate of mercury in the form of a black powder.

It ſeparates the volatile alkali in ſalt ammoniac from the acid, when it is mixed with a warm ſolution of that ſalt. But it does not ſeparate an acid from a calcarious earth, nor does it induce the leaſt change upon lime-water.

Lastly, when a dram of it is digeſted with an ounce of water in a bottle for ſome hours, it does not make any the leaſt change in the water. The *magneſia*, when dried, is found to have gained ten grains; but it neither effervesces with acids, nor does it ſenſibly affect lime-water.

OBSERVING *magneſia* to loſe ſuch a remarkable proportion of its weight in the fire, my next attempts were directed to the inveſtigation of this volatile part, and, among other experiments, the following ſeemed to throw ſome light upon it.

THREE ounces of *magneſia* were diſtilled in a glaſs retort and receiver, the fire being gradually increaſed until the *magneſia* was obſcurely red hot. When all was cool, I found only five drams of a whitiſh water in the receiver, which had a faint ſmell of the ſpirit of hartſhorn, gave a green colour to the juice of violets, and rendered the ſolutions of corroſive ſublimate and of ſilver very ſlightly turbid. But it did not ſenſibly efferveſce with acids.

THE *magneſia*, when taken out of the retort, weighed an ounce, three drams, and thirty grains, or had loſt more than the half of its weight. It ſtill efferveſced pretty briſkly with acids, though not ſo ſtrongly as before this operation.

THE fire ſhould have been raiſed here to the degree requiſite for the perfect calcination of *magneſia*. But, even from this imperfect experiment, it is evident, that, of the volatile parts contained in that powder, a ſmall proportion only is water; the reſt cannot, it ſeems, be retained in veſſels, under a viſible form. Chemiſts have often obſerved, in their diſtillations, that part of a body has vaniſhed from their ſenſes, notwithſtanding the utmoſt care to retain it; and they have always found, upon further inquiry, that ſubtile part to be air, which having been impriſoned in the body, under a ſolid form, was ſet free and rendered fluid and elaſtic by the fire. We may therefore ſafely conclude, that the volatile matter, loſt in the calcination of *magneſia*, is moſtly air; and hence the calcined *magneſia* does not emit air, or make an efferveſcence, when mixed with acids.

THE water, from its properties,

ſeems to contain a ſmall portion of volatile alkali, which was probably formed from the earth, air, and water, or from ſome of theſe combined together; and perhaps alſo from a ſmall quantity of inflammable matter which adhered accidentally to the *magneſia*. Whenever chemiſts meet with this ſalt, they are inclined to aſcribe its origin to ſome animal or putrid vegetable ſubſtance; and this they have always done, when they obtained it from the calcarious earths, all of which afford a ſmall quantity of it. There is, however, no doubt that it can ſometimes be produced independently of any ſuch mixture, ſince many freſh vegetables and tartar afford a conſiderable quantity of it. And how can it, in the preſent inſtance, be ſuppoſed, that any animal, or vegetable matter adhered to the *magneſia*, while it was diſſolved by an acid, ſeparated from this by an alkali, and waſhed with ſo much water?

Two drams of *magneſia* were calcined in a crucible, in the manner deſcribed above, and thus reduced to two ſcruples and twelve grains. This calcined *magneſia* was diſſolved in a ſufficient quantity of ſpirit of vitriol, and then again ſeparated from the acid by the addition of an alkali, of which a large quantity is neceſſary for this purpoſe. The *magneſia* being very well waſhed and dried, weighed one dram and fifty grains. It efferveſced violently, or emitted a large quantity of air, when thrown into acids; formed a red powder when mixed with a ſolution of ſublimate; ſeparated the calcarious earths from an acid, and ſweetened lime water: And had thus recovered all thoſe properties which it had but juſt now loſt by calcination. Nor had it only recovered its original properties, but acquired beſides an addition of weight nearly equal to what had been loſt in the fire; and, as it is found to efferveſce with acids, part of the addition muſt certainly be air.

This air ſeems to have been furniſhed by the alkali from which it was ſeparated by the acid; for Dr Hales has clearly proved, that alkaline ſalts contain a large quantity of fixed air, which they emit in great abundance when joined to a pure acid. In the preſent caſe, the alkali is really joined to an acid, but without any viſible emiſſion of air; and yet the air is not retained in it: For the neutral ſalt, into which it is converted, is the ſame in quantity, and in every other reſpect, as if the acid employed had not been previouſly ſaturated with *magneſia*, but offered to the alkali in its pure ſtate, and had driven the air out of it in their conflict. It ſeems therefore evident, that the air was forced from the alkali by the acid, and lodged itſelf in the *magneſia*.

THESE conſiderations led me to try a few experiments, whereby I might know what quantity of air is expelled from an alkali, or from *magneſia*, by acids.

Two drams of a pure fixed alkaline

ſalt, and an ounce of water, were put into a Florentine flaſk, which, together with its contents, weighed two ounces and two drams. Some oil of vitriol diluted with water was dropt in, until the ſalt was exactly ſaturated; which it was found to be, when two drams, two ſcruples, and three grains of this acid had been added. The phial with its contents now weighed two ounces, four drams, and fifteen grains. One ſcruple, therefore, and eight grains were loſt during the ebullition, of which a trifling portion may be water, or ſomething of the ſame kind. The reſt is air.

THE celebrated Homberg has attempted to eſtimate the quantity of ſolid ſalt contained in a determined portion of the ſeveral acids. He ſaturated equal quantities of an alkali with each of them ; and, obſerving the weight which the alkali had gained, after being perfectly dried, took this for the quantity of ſolid ſalt contained in that ſhare of the acid which performed the ſaturation. But we learn from the above experiment, that his eſtimate was not accurate; becauſe the alkali loſes weight as well as gains it.

Two drams of *magneſia*, treated exactly as the alkali in the laſt experiment, were juſt diſſolved by four drams, one ~~ſcruple, and~~ ſeven grains of the ſame acid liquor, and loſt one ſcruple and ſixteen grains by the ebullition.

Two drams of *magneſia* were reduced, by the action of a violent fire, to two ſcruples and twelve grains, with which the ſame proceſs was repeated as in the two laſt experiments; four drams, one ſcruple, and two grains of the ſame acid were required to compleat the ſolution, and no weight was loſt in the experiment.

As in the ſeparation of the volatile from the fixed parts of bodies, by means of heat, a ſmall quantity of the latter is generally raiſed with the former; ſo the air and water, originally contained in the *magneſia*, and afterwards diſſipated by the fire, ſeem to have carried off a ſmall part of the fixed earth of this ſubſtance. This is probably the reaſon, why calcined *magneſia* is ſaturated with a quantity of acid, ſomewhat leſs than what is required to diſſolve it before calcination: And the ſame may be aſſigned as one cauſe which hinders us from reſtoring the whole of its original weight by ſolution and precipitation.

I TOOK care to dilute the vitriolic acid, in order to avoid the heat and ebullition which it would otherwiſe have excited in the water; and I choſe a Florentine flaſk, on account of its lightneſs, capacity, and ſhape, which is peculiarly adapted to the experiment; for the vapours raiſed by the ebullition circulated for a ſhort time thro' the wide cavity of the phial, but were ſoon collected upon its ſides, like dew, and none of them ſeemed to reach the neck, which continued perfectly dry to the end of the experiment.

We now perceive the reaſon why crude and calcined *magneſia*, which differ in many reſpects from one another, agree however in compoſing the ſame kind of ſalt, when diſſolved in any particular acid; for the crude *magneſia* ſeems to differ from the calcined chiefly by containing a conſiderable quantity of air, which air is unavoidably diſſipated and loſt during the diſſolution.

From our experiments, it ſeems probable, that the increaſe of weight which ſome metals acquire, by being firſt diſſolved in acids, and then ſeparated from them again by alkalis, proceeds from air furniſhed by the alkalis. And that, in the *aurum fulminans*, which is prepared by the ſame means, this air adheres to the gold in ſuch a peculiar manner, that, in a moderate degree of heat, the whole of it recovers its elaſticity in the ſame inſtant of time; and thus, by the violent ſhock which it gives to the air around, produces the loud crack or fulmination of this powder. Thoſe who will imagine the exploſion of ſuch a minute portion of fixed air, as can reſide in the *aurum fulminans*, to be inſufficient for the exceſſive loudneſs of the noiſe, will conſider, that it is not a large quantity of motion communicated to the air, but rather a ſmart ſtroke, which produces ſound; and that the exploſion of but a few particles of fixed air may be capable of cauſing a loud noiſe, provided they all recover their ſpring ſuddenly, and in the ſame inſtant.

The above experiments lead us alſo to conclude, that volatile alkalis, and the common abſorbent earths, which loſe their air by being joined to acids, but ſhew evident ſigns of their having recovered it, when ſeparated from them by alkalis, received it from theſe alkalis which loſt it in the inſtant of their joining with the acid.

The following are a few experiments upon three of the abſorbent earths, made in order to compare them with one another, and with *magneſia*.

Suspecting that *magneſia* might poſſibly be no other than a common calcarious earth, which had changed its nature, by having been previouſly combined with an acid, I ſaturated a ſmall quantity of chalk with the muriatic acid, ſeparated the acid from it again by means of a fixed alkali, and carefully waſhed away the whole of the ſalt.

THE chalk when dried was not found to have ſuffered any alteration; for it efferveſced with the vitriolic acid, but did not diſſolve in it; and, when expoſed to a violent fire, was converted into a quick-lime, in all reſpects ſimilar to that obtained from common chalk.

IN another experiment of the ſame kind, I uſed the vitriolic acid with the ſame event.

ANY calcarious matter reduced to a fine powder, and thrown into a warm ſolution of alum, immediately raiſes a briſk efferveſcence. But the powder is not diſſolved; it is rather increaſed in bulk: And if the addition be repeated until it is no longer accompanied with efferveſcence, the liquor loſes all taſte of the alum, and yields only a very light cloud upon the admixture of an alkali.

From this experiment we learn, that acids attract the calcarious earths more ſtrongly than they do the earth of alum; and as the acid in this ſalt is exactly the ſame with the vitriolic, it compoſes with the calcarious earth a neutral ſubſtance, which is very difficultly ſoluble in water, and therefore falls down to the bottom of the veſſel along with the earth of alum which is deprived of its acid. The light cloud formed by the alkali proceeds from the minute portion of the calcarious compound which ſaturates the water.

The earth of animal bones, when reduced to a fine powder, and thrown into a diluted vitriolic acid, gradually abſorbs the acid in the ſame manner as the calcarious earths, but without any remarkable efferveſcence. When it is added to the nitrous or to the muriatic acid, it is ſlowly diſſolved. The compound liquor thence produced, is extremely acrid, and ſtill changes the colour of the juice of violets to a red, even after it is fully ſaturated with the abſorbent. Diſtilled vinegar has little or no effect upon this earth; for, after a long digeſtion, it ſtill retains its ſour taſte, and gives only a light cloud upon the addition of an alkali.

By dropping a diſſolved fixed alkali into a warm ſolution of alum, I obtained the earth of this ſalt, which, after being well waſhed and dried, was found to have the following properties:

It is diſſolved in every acid, but very ſlowly, unleſs aſſiſted by heat. The ſeveral ſolutions, when thoroughly ſaturated, are all aſtringent with a ſlight degree of an acid taſte, and they alſo agree with a ſolution of alum in this, that they give a red colour to the infuſion of turnſol.

Neither this earth, nor that of animal bones, can be converted into quicklime by the ſtrongeſt fire, nor do they ſuffer any change worth notice. Both of them ſeem to attract acids but weakly, and to alter their properties leſs when united to them than the other abſorbents.

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## PART II.

IN reflecting afterwards upon theſe experiments, an explication of the nature of lime offered itſelf, which ſeemed to account, in an eaſy manner, for moſt of the properties of that ſubſtance.

IT is ſufficiently clear, that the calcarious earths in their native ſtate, and that the alkalis and *magneſia* in their ordinary condition, contain a large quantity of fixed air, and this air certainly adheres to them with conſiderable force, ſince a ſtrong fire is neceſſary to ſeparate it from *magneſia*, and the ſtrongeſt is not ſufficient to expel it entirely from fixed alkalis, or take away their power of effervefcing with acid ſalts.

These conſiderations led me to conclude, that the relations between fixed air and alkaline ſubſtances was ſomewhat ſimilar to the relation between theſe and acids ; that as the calcarious earths and alkalis attract acids ſtrongly, and can be ſaturated with them, ſo they alſo attract fixed air, and are in their ordinary ſtate ſaturated with it ; and, when we mix an acid with an alkali or with an abſorbent earth, that the air is then ſet at liberty, and breaks out with violence ; becauſe the alkaline body attracts it more weakly than it does the acid, and becauſe the acid and air cannot both be joined to the ſame body at the ſame time.

I ALSO imagined, that, when the calcarious earths are expoſed to the action of a violent fire, and are thereby converted into quick-lime, they ſuffer no other change in their compoſition than the loſs of a ſmall quantity of water and of their fixed air. The remarkable acrimony which we perceive in them after this proceſs, was not ſuppoſed to proceed from any additional matter received in the fire, but ſeemed to be an eſſential property of the pure earth, depending on an attraction for thoſe ſeveral ſubſtances which it then became capable of corroding or diſſolving, which attraction had been inſenſible as long as the air adhered to the earth, but diſcovered itſelf upon the ſeparation.

THIS ſuppoſition was founded upon an obſervation of the moſt frequent conſequences of combining bodies in chymiſtry. Commonly when we join two bodies together, their acrimony or attraction for other ſubſtances becomes immediately either leſs perceivable or entirely inſenſible; although it was ſufficiently ſtrong and remarkable before their union, and may be rendered evident again by disjoining them. A neutral ſalt, which is compoſed of an acid and alkali, does not poſſeſs the acrimony of either of its conſtituent parts.

It can eaſily be ſeparated from water, has little or no effect upon metals, is incapable of being joined to inflameable bodies, and of corroding and diſſolving animals and vegetables; ſo that the attraction both of the acid and alkali for theſe ſeveral ſubſtances ſeems to be ſuſpended till they are again ſeparated from one another.

CRUDE lime was therefore conſidered as a peculiar acrid earth rendered mild by its union with fixed air: And quick-lime, as the ſame earth, in which, by having ſeparated the air, we diſcover that acrimony or attraction for water, for animal, vegetable, and for inflammable ſubſtances.

THAT the calcarious earths really

loſe a large quantity of air when they are burnt to quick-lime, ſeems ſufficiently proved by an experiment of Mr Margraaf *, an exceedingly accurate and judicious chymiſt. He ſubjected eight ounces of oſteocolla to diſtillation in an earthern retort, finiſhing his proceſs with the moſt violent fire of a reverberatory, and caught in the receiver only two drams of water, which by its ſmell and properties ſhewed itſelf to be ſlightly alkaline. He does not tell us the weight of the oſteocolla remaining in the retort, and only ſays, that it was converted into quick-lime; but, as no calcarious earth can be converted into quick-lime, or bear the heat which he applied without loſing above a third of

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* Mem. de l'Acad. de Berlin. an. 1748, p. 57.

its weight, we may ſafely conclude, that the loſs in his experiment was proportional, and proceeded chiefly from the diſſipation of fixed air.

According to our theory, the relation of the calcarious earth to air and water appeared to agree with the relation of the ſame earth to the vitriolic and vegetable acids. As chalk for inſtance has a ſtronger attraction for the vitriolic than for the vegetable acid, and is diſſolved with more difficulty when combined with the firſt, than when joined to the ſecond: So it alſo attracts air more ſtrongly than water, and is diſſolved with more difficulty when ſaturated with air, than when compounded with water only.

A calcarious earth deprived of its air, or in the ſtate of quick-lime, greedily abſorbs a conſiderable quantity of water, becomes ſoluble in that fluid, and is then ſaid to be ſlaked; but, as ſoon as it meets with fixed air, it is ſuppoſed to quit the water and join itſelf to the air, for which it has a ſuperior attraction, and is therefore reſtored to its firſt ſtate of mildneſs and inſolubility in water.

When ſlaked lime is mixed with water, the fixed air in the water is attracted by the lime, and ſaturates a ſmall portion of it, which then becomes again incapable of diſſolution, but part of the remaining ſlaked lime is diſſolved and compoſes lime-water.

If this fluid be expoſed to the open air, the particles of quick-lime which are neareſt the ſurface gradually attract the particles of fixed air which float in the atmoſphere. But at the ſame time that a particle of lime is thus ſaturated with air, it is alſo reſtored to its native ſtate of mildneſs and inſolubility; and as the whole of this change muſt happen at the ſurface, the whole of the lime is ſucceſſively collected there under its original form of an inſipid calcarious earth, called the cream or cruſts of lime-water.

When quick-lime itſelf is expoſed to the open air, it abſorbs the particles of water and of fixed air which come within its ſphere of attraction, as it meets with the firſt of theſe in greateſt plenty, the greateſt part of it aſſumes the form of ſlaked lime; the reſt is reſtored to its original ſtate; and, if it be expoſed for a ſufficient length of time, the whole of it is gradually ſaturated with air, to which the water as gradually yields its place.

We have already ſhewn by experiment, that *magneſia alba* is a compound of a peculiar earth and fixed air. When this ſubſtance is mixed with lime-water, the lime ſhews a ſtronger attraction for fixed air than that of the earth of *magneſia*; the air leaves this powder to join itſelf to the lime. And as neither the lime when ſaturated with air, nor the *magneſia* when deprived of it, are ſoluble in water, the lime water becomes perfectly pure and inſipid, the lime which it contained being mixed with the *magneſia*. But if the *magneſia* be deprived of air by calcination before it is mixed with the lime-water, this fluid ſuffers no alteration.

If quick-lime be mixed with a diſſolved alkali, it likeways ſhews an attraction for fixed air ſuperior to that of the alkali. It robs this ſalt of its air, and thereby becomes mild itſelf, while the alkali is conſequently rendered more corroſive, or diſcovers its natural degree of acrimony or ſtrong attraction for water, and for bodies of the inflameable, and of the animal and vegetable kind; which attraction was leſs perceivable as long as it was ſaturated with air. And the volatile alkali, when deprived of its air, beſides this attraction for various bodies, diſcovers likeways its natural degree of volatility, which was formerly ſomewhat repreſſed by the air adhering to it, in the ſame manner as it is repreſſed by the addition of an acid.

This account of lime and alkalis recommended itſelf by its ſimplicity, and by affording an eaſy ſolution of many *phœnomena*, but appeared upon a nearer view to be attended with conſequences that were ſo very new and extraordinary, as to render ſuſpicious the principles from which they were drawn.

I reſolved however to examine, in a particular manner, ſuch of theſe conſequences as were the moſt unavoidable, and found, the greateſt number of them might be reduced to the following propoſitions :

I. If we only ſeparate a quantity of air from lime and alkalis, when we render them cauſtic they will be found to loſe part of their weight in the operation, but will ſaturate the ſame quantity of acid as before, and the ſaturation will be performed without effervefcence.

II. If quick-lime be no other than a calcarious earth deprived of its air, and whoſe attraction for fixed air is ſtronger than that of alkalis, it follows, that, by adding to it a ſufficient quantity of alkali ſaturated with air, the lime will recover the whole of its air, and be entirely reſtored to its original weight and condition : And, it alſo follows, that the earth ſeparated from lime-water by an alkali, is the lime which was diſſolved in the water now reſtored to its original mild and inſoluble ſtate.

III. If it be ſuppoſed that ſlaked lime does not contain any parts which are more fiery, active, or ſubtile than others, and by which chiefly it communicates its virtues to water; but that it is an uniform compound of lime and water; it follows, that, as part of it can be diſſolved in water, the whole of it is alſo capable of being diſſolved.

IV. If the acrimony of the cauſtic alkali does not depend on any part of the lime adhering to it, a cauſtic or ſoap-ley will conſequently be found to contain no lime, unleſs the quantity of lime employed in making it were greater than what is juſt ſufficient to extract the whole air of the alkali; for then as much of the ſuperfluous quick-lime might poſſibly be diſſolved by the ley as would be diſſolved by pure water, or the ley would contain as much lime as lime-water does.

V. We have ſhewn in the former experiments, that abſorbent earths loſe their air when they are joined to an acid; but recover it, if ſeparated again from that acid by means of an ordinary alkali: The air paſſing from the alkali to the earth, at the ſame time that the acid paſſes from the earth to the alkali.

If the cauſtic alkali therefore be deſtitute of air, it will ſeparate *magneſia* from an acid under the form of a *magneſia* free of air, or which will not efferveſce with acids; and the ſame cauſtic alkali will alſo ſeparate a calcarious earth from acids, under the form of a calcarious earth deſtitute of air, but ſaturated with water, or under the form of ſlaked lime.

THESE were all neceſſary concluſions from the above ſuppoſitions. Many of them appeared too improbable to deſerve any further attention: Some, however, I found upon reflection were already ſeconded by experience. Thus Hoffman has obſerved, that quick-lime does not efferveſce with ſpirit of vitriol *; and it is well known that the cauſtic ſpirit of urine, or of ſalt ammoniac, does not emit air, when mixed with acids. This conſideration excited my curioſity, and determined me to inquire into the truth of them all by way

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* Hoff. Op. T. iv. p. 480.

of experiment. I therefore engaged myſelf in a ſet of trials; the hiſtory of which is here ſubjoined. Some new facts are likeways occaſionally mentioned; and here it will be proper to inform the reader, that I have never mentioned any without ſatisfying myſelf of their truth by experiment, though I have ſometimes taken the liberty to neglect deſcribing the experiments when they ſeemed ſufficiently obvious.

Desiring to know how much of an acid a calcarious earth will abſorb, and what quantity of air is expelled during the diſſolution, I ſaturated two drams of chalk with diluted ſpirit of ſalt, and uſed the Florentine flaſk, as related in a ſimilar experiment upon *magneſia*. Seven drams and one grain of the acid finiſhed the diſſolution, and the chalk loſt two ſcruples and eight grains of air.

THIS experiment was neceſſary before the following, by which I propoſed to inquire into the truth of the firſt propoſition, ſo far as it relates to quick-lime.

Two drams of chalk were converted into a perfect quick-lime, and loſt two ſcruples and twelve grains in the fire. This quick-lime was ſlaked or reduced to a milky liquor with an ounce of water, and then diſſolved in the ſame manner, and with the ſame acid, as the two drams of chalk in the preceeding experiment. Six drams, two ſcruples, and fourteen grains of the acid finiſhed the ſaturation without any ſenſible efferveſcence or loſs of weight.

It therefore appears from theſe experiments, that no air is ſeparated from quick-lime by an acid, and that chalk ſaturates nearly the ſame quantity of acid after it is converted into quick-lime as before.

With reſpect to the ſecond propoſition, I tried the following experiments:

A piece of perfect quick-lime made from two drams of chalk, and which weighed one dram and eight grains, was reduced to a very fine powder, and thrown into a filtrated mixture of an ounce of a fixed alkaline ſalt and two ounces of water. After a ſlight digeſtion, the powder being well waſhed and dried, weighed one dram and fifty eight grains. It was ſimilar in every trial to a fine powder of ordinary chalk, and was therefore ſaturated with air which muſt have been furniſhed by the alkali.

A dram of pure ſalt of tartar was diſſolved in fourteen pounds of lime-water, and the powder thereby precipitated, being carefully collected and dried, weighed one and fifty grains. When expoſed to a violent fire, it was converted into a true quick-lime, and had every other quality of a calcarious earth.

This experiment was repeated with

the volatile alkali, and alſo with the foſſil or alkali of ſea-ſalt, and exactly with the ſame event.

The third propoſition had leſs appearance of probability than the foregoing; but, as an accurate experiment was the only teſt of its truth, I reduced eight grains of perfect quick-lime made of chalk, to an exceedingly ſubtile powder, by ſlaking it in two drams of diſtilled water boiling hot, and immediately threw the mixture into eighteen ounces of diſtilled water in a flaſk. After ſhaking it, a light ſediment, which floated through the liquor, was allowed to ſubſide; and this, when collected with the greateſt care, and dryed, weighed, as near as I could gueſs, one third of a grain. The water taſted ſtrongly of the lime, had all the qualities of lime-water, and yielded twelve grains of precipitate, upon the addition of ſalt of tartar. In repeating this experiment, the quantity of ſediment was ſometimes leſs than the above, and ſometimes amounted to half a grain. It conſiſted partly of an earth which effervеſced violently with *aqua fortis*, and partly of an ochry powder, which would not diſſolve in that acid. The ochry powder, as it uſually appears in chalk to the eye, in the form of veins running through its ſubſtance, muſt be conſidered only as an accidental or foreign admixture; and, with reſpect to the minute portion of alkaline earth which compoſed the remainder of the ſediment, it cannot be ſuppoſed to have been originally different from the reſt, and incapable, from its nature, of being converted into quick-lime, or of being diſſolved in water; it ſeems rather to have conſiſted of a ſmall part of the chalk in its mild ſtate, or ſaturated with air, which had either remained, for want of a ſufficient fire to drive it out entirely, or had been furniſhed by the diſtilled water.

I indeed expected to ſee a much larger quantity of ſediment produced from the lime, on account of the air which water conſtantly contains; and, with a view to know whether water retains its air when fully ſaturated with lime, a lime-water was made as ſtrong as poſſible: Four ounces of which were placed under the receiver of an air-pump, together with four ounces of common water in a phial of the ſame ſize; and, upon exhauſting the receiver, without heating the phials, the air aroſe from each in nearly the ſame quantity: From whence it is evident, that the air, which quick-lime attracts, is of a different kind from that which is mixed with water. And that it is alſo different from common elaſtic air, is ſufficiently proved by daily experience; for lime-water, which ſoon attracts air, and forms a cruſt when expoſed in open and ſhallow veſſels, may be preſerved, for any time, in bottles which are but ſlightly corked, or cloſed in ſuch a manner as would allow free acceſs to elaſtic air, were a vacuum formed in the bottle. Quick-lime therefore does not attract air when in its moſt ordinary form, but is capable of being joined to one particular ſpecies only, which is diſperſed through the atmoſphere, either in the ſhape of an exceedingly ſubtile powder, or more probably in that of an elaſtic fluid. To this I have given the name of fixed air, and perhaps very improperly; but I thought it better to uſe a word already familiar in philoſophy, than to invent a new name, before we be more fully acquainted with the nature and properties of this ſubſtance, which will probably be the ſubject of my further inquiry.

It is, perhaps, needleſs to mention here, that the calcarious ſubſtances uſed in making the above experiments ſhould be of the pureſt kind, and burnt with the utmoſt violence of heat, if we would be ſure of converting them into perfect quick-lime. I therefore made uſe of chalk burnt in a ſmall covered crucible with the fierceſt fire of a blackſmith's forge, for half an hour, and found it neceſſary to employ, for this purpoſe, a crucible of the Auſtrian kind, which reſemble black lead; for, if any calcarious ſubſtance be heated to ſuch a degree in an ordinary or Heſſian crucible, the whole of it is melted down, together with part of the veſſel, into glaſs.

I NOW prepared to inquire into the properties of the cauſtic alkali; in order to which, I made a cauſtic or ſoap-ley in the following manner:

TWENTY-SIX ounces of very ſtrong

quick-lime made of chalk, were ſlaked or reduced to a ſort of fluid paſte, with eleven pounds of boiling water, and then mixed in a glaſs veſſel with eighteen ounces of a pure fixed alkaline ſalt, which had been firſt diſſolved in two pounds and a half of water. This mixture was ſhaken frequently for two hours, when the action of the lime upon the alkali was ſuppoſed to be over, and nothing remained but to ſeparate them again from one another. I therefore added 12 pounds of water, ſtirred up the lime, and, after allowing it to ſettle again, poured off as much of the clear ley as poſſible.

The lime and alkali were mixed together under the form of a very thick milky liquor or fluid paſte; becauſe they are thus kept in perpetual contact and equal mixture until they have acted ſufficiently upon one another: Whereas, in the common way of uſing a larger quantity of water, the lime lies for the moſt part at bottom, and, tho' ſtirred up ever ſo often, cannot exert its influence ſo fully upon the alkali, which is uniformly diffuſed thro' every part of the liquor.

The above ley was found upon trial to be ſaturated by acids, without the leaſt efferveſcence or diminution of weight.

It was now proper to examine whether the alkali ſuffered any loſs in becoming cauſtic, which I propoſed to attempt by aſcertaining the ſtrength of the ley, or the quantity of ſalt which a given portion of it contained; from which, by computation, ſome imperfect knowledge might be obtained of the quantity of cauſtic produced from the eighteen ounces of mild ſalt.

I therefore evaporated ſome of my ley; but ſoon perceived that no certain judgment could be formed of its ſtrength in this way; becauſe it always abſorbed a conſiderable quantity of air during the evaporation, and the dried ſalt made a pretty briſk effervеſcence with acids, ſo that the ley appeared ſtronger than it really was; and yet, upon proceeding in the eſtimate from this rude and unfair trial, it appeared that the ſalt had loſt above a ſixth in becoming cauſtic, and the quantity of acid ſaturated by two drams of it was to the quantity of acid ſaturated by two drams of ſalt of tartar, nearly as ſix to five.

These experiments are therefore agreeable to that part of the ſecond propoſition which relates to the cauſtic alkali.

Upon farther examining what changes the alkali had undergone, I found that the ley gave only an exceeding faint milky hue to lime-water; becauſe the cauſtic alkali wants that air by which ſalt of tartar precipitates the lime. When a few ounces of it were expoſed in an open ſhallow veſſel for four and twenty hours, it imbibed a ſmall quantity of air, and made a ſlight efferveſcence with acids. After a fortnight's expoſure in the ſame manner, it became entirely mild, efferveſced as violently with acids, and had the ſame effect upon lime-water, as a ſolution of an ordinary alkali. It likeways agrees with lime-water in this reſpect, that it may be kept in cloſe veſſels, or even in bottles which are but ſlightly covered, for a conſiderable time, without abſorbing a ſenſible quantity of air.

In order to know how much lime it contained, I evaporated ten ounces in a ſmall ſilver diſh over a lamp, and melted the ſalt, after having diſſipated the water *.

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* This evaporation was performed in a ſilver diſh, on account of the acrimony of the ſalt; which is ſo very great, that, having once evaporated a part of the ſame ley in a bowl of En-

THE cauſtic thus produced, was diſſolved again in a ſmall quantity of water, and depoſited a trifling portion of ſediment, which I imagined at firſt to be lime; but, finding that it could eaſily be diſſolved in a little more water, concluded it to be a vitriolated tartar, which always accompanies the fixed alkali of vegetables.

I then ſaturated the ſolution of the cauſtic ſalt with ſpirit of vitriol, expecting thus to detect the lime; becauſe that acid precipitates a calcarious earth from its ordinary ſolutions. During the ſaturation, a large quantity of

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gliſh earthen or ſtone ware, and melted the cauſtic with a gentle heat, it corroded and diſſolved a part of the bowl, and left the inſide of it pitted with ſmall holes.

white powder was formed; but this likeways turned out to be a vitriolated tartar, which had appeared in the form of a powder, becauſe there was not enough of water in the mixture to diſſolve it.

LASTLY, I expoſed a few ounces of the ley in an open ſhallow veſſel, ſo long that the alkali loſt the whole of its cauſticity, and ſeemed entirely reſtored to the ſtate of an ordinary fixed alkali; but it did not however depoſite a ſingle atom of lime. And, to aſſure myſelf that my cauſtic ley was not of a ſingular kind, I repeated the ſame experiments with an ordinary ſoap-ley, and with one made by mixing one part of a pure fixed alkaline ſalt with three parts of common ſtone-lime freſh ſlaked and ſifted; nor could I diſcover any lime in either. The firſt of theſe contained a ſmall quantity of brimſtone, and was far from being perfectly cauſtic, for it made a pretty briſk effervefcence with acids; but the laſt was ſo entirely deprived of its air, that it did not diminiſh in the leaſt the tranſparency of lime-water.

THESE experiments ſeem therefore to ſupport the fourth propoſition, and to ſhew that the cauſtic alkali does not contain any lime.

As it ſeems probable, from the quickneſs and eaſe wherewith the alkali was rendered cauſtic, that more lime had been employed than what was juſt ſufficient to extract the whole of its air, we are ſurpriſed to find that little or none of the ſuperfluous quick-lime was diſſolved by the water. But this *phænomenon* will become leſs ſurprizing, by comparing it with ſome ſimilar inſtances in chemiſtry. Water may be made to depoſite a ſalt, by the admixture of a ſubſtance which it attracts more ſtrongly than it does that ſalt; ſuch as ſpirit of wine; and quick-lime itſelf may be ſeparated from water upon the ſame principle: For if that ſpirit is added to an equal quantity of lime-water, the mixture becomes turbid, and depoſites a ſediment, which, when ſeparated and diſſolved again in diſtilled water, compoſes lime-water. We may therefore refer the above *phænomenon*, with reſpect to the ley, to the ſame cauſe with theſe, and ſay, that the water did not diſſolve the lime, becauſe it already contained a cauſtic alkali, for which it has a ſuperior attraction.

I alſo rendered the volatile alkali cauſtic, in order to examine what change it ſuffered in the operation, and obtained an exceedingly volatile and acrid ſpirit, which neither effervefced with acids, nor altered in the leaſt the tranſparency of lime-water; and, altho' very ſtrong, was lighter than water, and floated upon it like ſpirit of wine.

I next inquired into the truth of the fifth propoſition, in the following manner:

Two drams of Epſom ſalt were diſſolved in a ſmall quantity of water, and thrown into two ounces of the cauſtic ley; the mixture inſtantly became thick, like a decoction of ſtarch or barley, by the *magneſia*, which was precipitated. I then added ſpirit of vitriol by degrees, until the mixture became perfectly clear, or the whole of the *magneſia* was again diſſolved; which happened without any efferveſcence or emiſſion of air.

HALF an ounce of chalk was diſſolved in ſpirit of ſalt, the quantity of which was ſo adjuſted, that the mixture was not acid in the leaſt degree; and the ſolution was thrown into twelve ounces of the cauſtic ley; which quantity I found, by experiment, to be ſufficient for precipitating almoſt the whole of the chalk. I now filtrated this turbid liquor, and laid the powder remaining in the paper upon a chalk ſtone, in order to draw as much of the water from it as poſſible, and thereby reduce it to the form of a more denſe and heavy powder, that it might ſubſide the more perfectly in the following part of the experiment. I then mixed it with about twenty ounces of pure water in a flaſk, and, after allowing the powder to ſubſide, poured off the water, which had all the qualities of lime-water. And I ſucceſſively converted eight waters more into lime-water, ſeven of theſe in the ſame quantity, and with the ſame management, as the firſt. The eighth was likeways in the ſame quantity; but I allowed it to remain with the chalk, and ſhook it frequently for two days. This, after being filtrated, formed a cream or cruſt upon its ſurface when expoſed to the air; changed the colour of the juice of violets into green; ſeparated an orange-coloured powder from a ſolution of corroſive ſublimate; became turbid upon the addition of an alkali; was entirely ſweetened by *magneſia;* and appeared ſo ſtrong to the taſte, that I could not have diſtinguiſhed it from ordinary lime-water. And when I threw ſome ſalt ammoniac into the lime which remained, the vapour of the volatile alkali immediately aroſe from the mixture.

In this experiment therefore the air is firſt driven out of the chalk by an acid, and then, in order to ſeparate this acid from it, we add an alkali which has been previouſly deprived of its air;

by which means the chalk itſelf is alſo obtained free of air, and in an acrid form, or in the form of ſlaked lime.

WE have alſo ſeveral proceſſes for obtaining the volatile alkali in a cauſtic form, which ſeem to be only ſo many methods of obtaining it in its pure ſtate, and free of fixed air. The firſt of theſe is the ſeparation of the alkali from an acid, merely by heat; an inſtance of which we have from Mr Margraaf *. He prepared from urine an ammoniacal ſalt, the acid of which is the baſis of the phoſphorus, and is of ſuch a peculiar nature, that it endures a red heat without being diſſipated. Sixteen ounces of the neutral ſalt were ſubjected by

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* Mem. de l'Acad. de Berlin, an. 1746, p. 87.

him to diſtillation. The acid remained in the retort, and he found in the receiver eight ounces of an alkaline ſpirit, which, he tells us, was extremely volatile, very much reſembling the ſpirit of ſalt ammoniac diſtilled with quicklime; and no cryſtals were formed in it, when expoſed to the cold air.

A cauſtic volatile alkali may alſo be obtained, by mixing ſalt ammoniac with half its weight of a cauſtic fixed alkali, or of *magneſia* which has been previouſly deprived of its air by fire; and then ſubmitting theſe mixtures to diſtillation: Or, merely by adding any ordinary volatile alkali to a proper quantity of a cauſtic ley; for, in this caſe, the air paſſes from the volatile to the fixed alkali, by a ſuperior attraction for the laſt, and, by a gentle heat, the compound yields a ſpirit ſimilar to that prepared from ſalt ammoniac and quicklime.

It is therefore probable, that, had we alſo a method of ſeparating the fixed alkali from an acid, without, at the ſame time, ſaturating it with air, we ſhould then obtain it in a cauſtic form; but I am not acquainted with an inſtance of this ſeparation in chymiſtry. There are two indeed, which, at firſt ſight, appear to be of this kind; theſe are the ſeparation of the fixed alkali from the nitrous acid by means of inflamed charcoal, in the proceſs for making *nitrum fixatum*, and of the ſame alkali, from vegetable acids merely by heat; but, upon examining the product of each proceſs, we find the alkali either fully or nearly ſaturated with air. In the firſt, either the charcoal or the acid, or both together, are almoſt wholly converted into air; a part of which is probably joined to the alkali. In the ſecond, the acid is not properly ſeparated, but rather deſtroyed by the fire: A conſiderable portion of it is converted into an inflameable ſubſtance; and we learn from Dr Hales, that the bodies of this claſs contain a large quantity of fixed air.

When we conſider that the attraction of alkalis for fixed air is weaker than that of the calcarious earths, and reflect upon the effects of heat in chymiſtry, we are led to imagine, that alkalis might be entirely deprived of their air, or rendered perfectly cauſtic, by a fire ſomewhat weaker than that which is ſufficient to produce the ſame change upon lime; but this opinion does not ſeem agreeable to experience.

The alkalis do, however, acquire ſome degree of cauſticity in a ſtrong fire, as appears from their being more eaſily united with ſpirit of wine after having been kept in fuſion for ſome time. For that fluid, which cannot be tinctured by a mild ſalt of tartar, will ſoon take a very deep colour from a few drops of a ſtrong cauſtic ley. The circumſtances which hinder us from rendering theſe ſalts perfectly cauſtic by heat, are their propenſity to diſſipation in the utmoſt violence of the fire, their extreme acrimony, and the imperfection of our common veſſels. For, before the heat becomes very intenſe, the alkalis either evaporate, or diſſolve a part of the crucibles in which they are contained, and often eſcape through their pores; which happens, eſpecially as ſoon as they have already acquired ſome degree of additional acrimony, by the loſs of part of their air.

The fuſion alſo, which they ſo readily undergo, is well known by chymiſts, as a ſtrong obſtacle to the ſeparation of the volatile from the fixed parts of a compound by fire. Accordingly, in ſeveral proceſſes, we are directed to add to the fuſible compound ſome porous ſubſtance which is incapable of fuſion, and will retain the whole in a ſpongy form, thereby to facilitate the diſſipation of the volatile parts.

In order to know whether an alkali would loſe a part of its air, and acquire a degree of cauſticity, when expoſed, with this precaution, to the action of a ſtrong fire, I mixed an ounce and a half of ſalt of tartar with three ounces of black lead, a ſubſtance of any the moſt unchangeable by chymical operations. This mixture I expoſed, for ſeveral hours, in a covered crucible, to a fire ſomewhat ſtronger than what is neceſſary to keep ſalt of tartar in fuſion. When allowed to cool, I found it ſtill in the form of a looſe powder; and taking out one half, I diluted it with water, and by filtration obtained a ley, which, when poured into a ſolution of white marble in *aqua fortis*, precipitated the marble under the form of a weak quick-lime: for the turbid mixture gave a green colour to the juice of violets, and threw up a cruſt like that of lime-water; and the precipitated powder collected and mixed with ſalt ammoniac immediately yielded the ſcent of the volatile alkali.

Lest it ſhould here be ſuſpected, that the alkaline qualities of this mixture, and of the precipitated marble were not owing to a lime into which the marble was converted, but to the alkali itſelf which was added, it is proper to obſerve, that I mixed ſo ſmall a proportion of the ley with the ſolution of marble, as made me ſure, from certain experiments, that the whole of the alkali was ſpent in performing the precipitation, and was conſequently converted into a neutral ſalt by attracting the acid. The properties therefore of the mixture can only be referred to a lime, as is indeed ſufficiently evident from the cruſt which is peculiar to lime-water.

I was therefore aſſured by this experiment, that an alkali does really loſe a part of its air, and acquire a degree of cauſticity, by the proper application of heat; but finding, by ſeveral trials, that the degree of cauſticity which it had thus acquired was but weak, and that the quick-lime produced in this experiment was exhauſted and rendered mild by a ſmall quantity of water, I expoſed the crucible, together with that half of the alkali which remained in it, to a ſtronger fire, in order to expel a larger quantity of air, and render it more remarkably cauſtic; but the whole of it was diſſipated by the force of the heat, and the black lead, which ſtill retained the form of a looſe and ſubtile powder, yielded little or nothing to water.

We learn then from the above experiment, the reaſon why the alkali newly obtained from the aſhes of vegetables is generally of the more acrid kinds of that ſalt. It never appears until the ſubject be converted into aſhes, and is ſuppoſed to be formed by the fire, and to be the reſult of a particular combination of ſome of the principles of the vegetable; one of which principles is air, which is contained in large quantity in all vegetable matters whatever. But, as ſoon as the ſmalleſt part of a vegetable is converted into aſhes, and an alkali is thus formed, this ſalt neceſſarily ſuffers a calcination, during which it is kept in a ſpongy form by the aſhes, and ſhews a very conſiderable degree of acrimony, if immediately applied to the body of an animal; but, if the aſhes are for any time expoſed to the air, or if we ſeparate the alkali from them by the addition of a large quantity of water and ſubſequent evaporation, the ſalt imbibes fixed air from the atmoſphere, and becomes nearly ſaturated with it: Though, even in this condition, it is generally more acrid than ſalt of tartar when this is prepared with a gentle heat.

BORAX has ſometimes been referred to the claſs of alkalis, on account of ſome reſemblance it bears to thoſe ſalts: But it has been demonſtrated by accurate experiments, that we ſhould rather conſider it as a neutral ſalt; that it is compoſed of an alkali and of a particular ſaline ſubſtance called the ſedative ſalt, which adheres to the alkali in the ſame manner as an acid, but can be ſeparated by the addition of any acid whatever, the added acid joining itſelf to the alkali in the place of the ſedative ſalt. As this conjunction of an acid with the alkali of borax happens without the leaſt efferveſcence, our principles lay us under a neceſſity of allowing that alkali to be perfectly free of air, which muſt proceed from its being incapable of union with fixed air and with the ſedative ſalt at the ſame time: Whence it follows, that, were we to mix the ſedative ſalt with an alkali ſaturated with air, the air would immediately be expelled, or the two ſalts in joining would produce an effervеſcence. This I found to be really the caſe upon making the trial, by mixing a ſmall quantity of the ſedative ſalt with an equal quantity of each of the three alkalis, rubbing the mixtures well in a mortar, and adding a little water. It is however proper in this place to obſerve, that, if the experiments be made in a different manner, they are attended with a ſingular circumſtance. If a ſmall quantity of the ſedative ſalt be thrown into a large proportion of a diſſolved fixed alkali, the ſedative ſalt gradually diſappears, and is united to the alkali without any effervеſcence; but, if the addition be repeated ſeveral times, it will at laſt be accompanied with a briſk effervefcence, which will become more and more remarkable, until the alkali be entirely ſaturated with the ſedative ſalt.

THIS *phœnomenon* may be explained by conſidering the fixed alkalis as not perfectly ſaturated with air: And the ſuppoſition will appear very reaſonable, when we recollect, that thoſe ſalts are never produced without a conſiderable degree of heat, which may eaſily be imagined to diſſipate a ſmall portion of ſo volatile a body as air. Now, if a ſmall quantity of the ſedative ſalt be thrown into an alkaline liquor, as it is very ſlowly diſſolved by water, its particles are very gradually mixed with the atoms of the alkali. They are moſt ſtrongly attracted by ſuch of theſe atoms as are deſtitute of air, and therefore join with them without producing an effervеſcence; or, if they expel a ſmall quantity of air from ſome of the ſalt, this air is at the ſame time abſorbed by ſuch of the contiguous particles as are deſtitute of it, and no effervеſcence appears until that part of the alkali, which was in a cauſtic form or deſtitute of air, be nearly ſaturated with the ſedative ſalt. But if, on the other hand, a large proportion of the ſedative ſalt be perfectly and ſuddenly mixed with the alkali, the whole or a large part of the air is as ſuddenly expelled.

In the ſame manner may we alſo explain a ſimilar *phænomenon*, which often preſents itſelf in ſaturating an alkali with the different acids : The effervefcence is lefs confiderable in the firſt additions of acid, and becomes more violent as the mixture approaches the point of ſaturation. This appears moſt evidently in making the *ſal diureticus* or regenerated tartar : The particles of the vegetable acid here employed being always diffuſed through a large quantity of water, are more gradually applied to thoſe of the alkali, and during the firſt additions are chiefly united to thoſe that are freeſt of air *.

THAT the fixed alkali, in its ordinary ſtate, is ſeldom entirely ſaturated with air, ſeems to be confirmed by the following experiment.

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* Boerh. operat. chym. proceſs. 76.

I expoſed a ſmall quantity of a pure vegetable fixed alkali to the air, in a broad and ſhallow veſſel, for the ſpace of two months; after which I found a number of ſolid cryſtals, which reſembled a neutral ſalt ſo much as to retain their form pretty well in the air, and to produce a conſiderable degree of cold when diſſolved in water. Their taſte was much milder than that of ordinary ſalt of tartar; and yet they ſeemed to be compoſed only of the alkali, and of a larger quantity of air than is uſually contained in that ſalt, and which had been attracted from the atmoſphere: For they ſtill joined very readily with any acid, but with a more violent effervеſcence than ordinary; and they could not be mixed with the ſmalleſt portion of vinegar, or of the ſedative ſalt, without emitting a ſenſible quantity of air.

As it now appeared that ſeveral alkaline ſubſtances have an attraction for fixed air, I tried a few experiments to learn the relative ſtrength of their ſeveral attractions.

TWENTY-FOUR grains of *magneſia* in fine powder were mixed with five ounces of the cauſtic ley in a ſmall phial, which was immediately corked and ſhaken frequently for four hours. The ley was then poured off, and the *magneſia* waſhed with repeated affuſions of water, and dried. It had loſt about the half of its weight, and, when reduced to a fine powder, was readily diſſolved by acids with an efferveſcence which was hardly perceivable: The alkali had therefore extracted its air. I alſo threw ſome freſh *magneſia* into the ley which had been poured off, and thereby rendered it perfectly mild and ſimilar to a ſolution of ſalt of tartar; ſo that it effervefced briſkly with acids.

WITH an ounce of the mild ſpirit of ſalt ammoniac, I mixed a dram of *magneſia* in very fine powder which had been previouſly deprived of its air by fire; and obſerving that the *magneſia* had a tendency to concrete into a ſolid maſs, I ſhook the phial very frequently. After ſome days the powder was increaſed to more than double its former bulk; and, when the phial was opened, the alkaline ſpirit emitted a moſt intolerably pungent ſmell. It likewiſe floated upon water, but was not perfectly cauſtic; for it ſtill yielded ſome air when mixed with acids, and alſo rendered lime-water turbid: Neither of which would probably have happened if I had uſed a greater quantity of *magneſia*, or had allowed the mixture to remain a longer time in the phial. I now waſhed out the whole of the mixture into a bowl, and dryed the *magneſia* until it loſt all ſmell of the alkali. It weighed a dram and fifty-eight grains, effervefced violently with acids, and therefore contained a large quantity of air, which had been drawn from the alkali by a ſtronger attraction.

Having formerly ſhewn, that *magneſia* ſaturated with air ſeparates an acid from a calcarious earth, which it is not able to do after being deprived of its air by fire; I now ſuſpected that the air was the cauſe of this ſeparation, becauſe I found that it was joined to the calcarious earth at the ſame time that the acid was joined to the earth of *magneſia;* and imagined, that a pure calcarious earth might poſſibly have a ſtronger attraction for acids than a pure earth of *magneſia.*

I therefore diſſolved two drams of *magneſia* in the marine acid, and thus obtained a compound of an acid and of the pure earth of this ſubſtance; for the air which was at firſt attached to it, was expelled during the diſſolution. I then added thirty grains of ſtrong quick-lime in exceeding fine powder, ſhook the mixture well, and filtrated it.

The powder remaining in the paper, after being well waſhed, was found to be a *magneſia*, which, as I expected, was deſtitute of air; for it was diſſolved by the vitriolic acid without efferveſcence, and the filtrated liquor contained the lime united to the acid; for, upon dropping ſpirit of vitriol into it, a white powder was immediately formed.

We muſt therefore acknowledge a ſtronger attraction between the calcarious earths and acids, than between theſe and *magneſia*: But how does it then happen, that, if *magneſia* ſaturated with air be mixed with a compound of acid and calcarious earth, theſe two laſt, which attract one another the moſt ſtrongly, do not remain united; but the acid is joined to the *magneſia*, and the calcarious earth to the air, which it attracts much more weakly than it does the acid? Is it becauſe the ſum of the forces which tend to join the *magneſia* to the acid, and the calcarious earth to the air, is greater than the ſum of the forces which tend to join the calcarious earth to the acid, and the *magneſia* to the air; and becauſe there is a repulſion between the acid and air, and between the two earths; or they are ſome how kept aſunder, in ſuch a manner as hinders any three of them from being united together?

The firſt part of this ſuppoſition is favoured by our experiments, which ſeem to ſhew a greater difference between the forces wherewith the calcarious earth and *magneſia* attract fixed air, than between thoſe which diſpoſe them to unite with the acid. The repulſions however hinted in the ſecond are perhaps more doubtful, though they are ſuggeſted in many other inſtances of decompoſition; but the bounds of my preſent purpoſe will not allow me to enter upon this ſubject, which is one of the moſt extenſive in chymiſtry.

We meet alſo with a difficulty with reſpect to the volatile alkali ſimilar to the above. Thus a calcarious earth that is pure or free of air has a much ſtronger attraction for acids than a pure volatile alkali, as is evident when we mix quick-lime with ſalt ammoniac; for the alkali is then immediately detached from the acid: And agreeably to this I found, upon the trial, that a pure or cauſtic volatile alkali does not ſeparate a calcarious earth from an acid. Yet, if we mix a mild volatile alkali, which is a compound of alkali and air, with a compound of acid and calcarious earth, theſe two laſt, which attract one another moſt ſtrongly, do not remain united; but the acid is joined to the alkali and the earth to the air, as happens in the precipitation of a calcarious earth from an acid, by means of the common or mild volatile alkali.

I remember likewiſe a parallel inſtance with regard to quick-ſilver. This metal has an attraction for the vitriolic acid, and, when joined to it, appears under the form of turbith mineral: But this attraction is weaker than that of the fixed alkali for the ſame acid; for if we mix a diſſolved ſalt of tartar with turbith mineral, the turbith is converted into a brown powder, and the alkali into vitriolated tartar; which change happens the ſooner, if the pure or cauſtic alkali is uſed. Yet, if to a compound of quick-ſilver and the nitrous acid, we add a compound of the fixed alkali and the vitriolic acid, or a vitriolated tartar, and digeſt the mixture with a ſtrong heat, the vitriolic acid does not remain with the alkali, but is joined to the quick-ſilver, which it attracts more weakly, compoſing with it a turbith mineral; while the alkali is joined to the nitrous acid, which it likeways attracts more weakly than it does the vitriolic, and is converted into ſalt-petre.

From ſome of the above experiments, it appears, that a few alterations may be made in the column of acids in Mr Geoffroy's table of elective attractions; and that a new column may be added to that table, according to the following ſcheme, where the alkaline ſubſtances are all conſidered as in their pure ſtate, and free of fixed air.

| Acids. | Fixed air. |
|---|---|
| Fixed alkali. | Calcarious earth. |
| Calcarious earth, | Fixed alkali. |
| Volatile alkali and *magneſia*, | *Magneſia.* |
| * * * * * * | Volatile alkali. |
| | * * * * |

At the foot of the firſt column ſeveral of the metals might follow, and after theſe the earth of alum; but, as

I don't know what number of the metals ſhould precede that earth, I have left it to be determined by further experience.

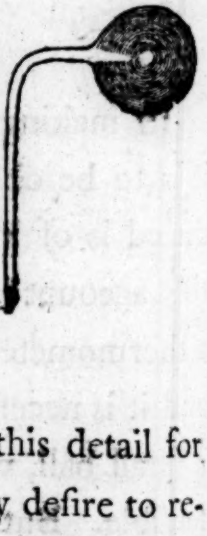
The volatile alkali and *magneſia* are placed in the ſame line of this column; becauſe their force of attraction ſeems pretty equal. When we commit a mixture of *magneſia* and ſalt ammoniac to diſtillation, the alkali ariſes and leaves the acid with the *magneſia;* becauſe this earth, by attracting the acid, repreſſes its volatility, and it ſeems alſo to diminiſh the coheſion of the acid and alkali, and to render them ſeparable by a gentle heat. If the *magneſia* be ſaturated with air, this likewiſe, on account of its volatile nature and attraction for the alkali, is driven up along with it, and makes it appear under a mild form; and in the ſame manner do the alkali and air ariſe from a mixture of ſalt ammoniac and of a crude calcarious earth.

## *Of the Cold produced by evaporating Fluids, and of ſome other means of producing Cold; by Dr* WILLIAM CULLEN.

A Young gentleman, one of my pupils, whom I had employed to examine the heat or cold that might be produced by the ſolution of certain ſubſtances in ſpirit of wine, obſerved to me, That, when a thermometer had been immerſed in ſpirit of wine, tho' the ſpirit was exactly of the temperature of the ſurrounding air, or ſomewhat colder; yet, upon taking the thermometer out of the ſpirit, and ſuſpending it in the air, the mercury in the thermometer, which was of Farenheit's conſtruction, always ſunk two or three degrees. This recalled to my mind ſome experiments and obſervations of M. de Mairan, to the ſame purpoſe; which I had read ſome time before. *See Diſſertation ſur la glace*, edit. 1749. *p.* 248, *et ſeq.* When I firſt read the experiments of M. de Mairan in the place referred to, I ſuſpected that water, and perhaps other fluids, in evapoporating, produced, or, as the phraſe is, generated ſome degree of cold. The above experiment of my pupil confirmed my ſuſpicion, and engaged me to verify it by a variety of new trials.

I began by repeating the experiment with ſpirit of wine; and found, when I had taken the utmoſt care to have the ſpirit exactly of the temperature of the air, that conſtantly however, upon taking the thermometer out of the ſpirit, the mercury ſunk ſeveral degrees, and indeed continued to ſink ſo long as the ball of the thermometer continued wet with the ſpirit of wine. I found alſo, when the ball began to dry, and the mercury to riſe again in the ſtem of the thermometer, that, if the ball was again dipped into the ſpirit, and immediately taken out, the mercury in the thermometer might be again obſerved to ſink; and that thus, by repeated dippings, the cold produced might be rendered very remarkable. The cold produced was alſo obſerved to be ſtill greater, when, between each dipping, the thermometer was moved very nimbly to and fro in the air; or if, while the ball was wet with ſpirit of wine, it was blown upon by a pair of bellows, or indeed if the air about the ball was otherwiſe any how put in motion. If any of theſe means for putting the air in motion are employed, the repeated moiſtenings of the ball of the thermometer may be performed by dipping it into the ſpirit of wine. But, when a certain degree of cold has been produced by a firſt dipping, that is apt to be diminiſhed by dipping again into the warmer ſpirit; and therefore the thermometer ought either to be dipped into the ſpirit, and taken out again very quickly, or, what is ſtill better, the ball of the thermometer ought to be moiſtened by a feather that has been dipped into the ſpirit of wine. By taking theſe methods, I have, by ſpirit of wine, made the mercury in the thermometer ſink from 44 degrees to below the freezing point; and, by employing ſome other fluids to be mentioned by and by, I have produced a ſinking of the thermometer much more conſiderable.

In making experiments of this kind, it is to be obſerved, that the cold produced is of very ſhort duration. On this account it is not proper to employ a thermometer incloſed in a glaſs tube; and it is neceſſary to employ one having a ſmall ball, which may render it more ſenſible. But, as a ſmall ball occaſions the diviſions of the ſcale to be the ſmaller, a thermometer filled with quick ſilver is not ſo proper for theſe experiments, as one filled with ſpirit of wine; having at the ſame time both a ſmall ball and a ſlender ſtem. What beſt of all ſhews the cold produced, and is indeed, with reſpect to ſeveral fluids, quite neceſſary, is an *air* thermometer. This too will be rendered more convenient, by having the upper part of the tube bent as in the figure annexed, ſo that the ball may be moiſtened without the liquor's running down upon the ſtem and ſcale.



I have entered into this detail for the ſake of thoſe who may deſire to repeat my experiments. Having now ſaid enough on the manner of making them, I go on to obſerve, that in this way I have examined a great variety of fluids. Such as,

The *quick-lime ſpirit of ſal. ammoniac*,
The *æther of Frobenius*,
The *nitrous æther*,
The *volatile tincture of ſulphur*,
*Spirit of wine*,
*Spirit of ſal. ammoniac* made with the
fixed *alkali*,
*Brandy*,
*Wine*,
*Vinegar.*
*Water*,
*Oil of turpentine*,
*Oil of mint*,
*Oil of pimento.*

By each of theſe employed to moiſten the ball of the thermometer, ſome degree of cold is produced. I dare not however at preſent determine exactly what is the ſinking of the thermometer produced by each. For this purpoſe, it would be neceſſary to repeat the trials often and with preciſely the ſame circumſtances at each time: Which I find to be very difficult. In the mean time I have endeavoured to give a notion of the comparative power of theſe fluids in producing cold, by the order in which I have ſet them down; having mentioned that fluid firſt which ſeemed to me to occaſion the greateſt ſinking of the thermometer, and the reſt follow in order as they ſeemed to occaſion leſs and leſs.

From the above enumeration I imagine it will appear, that the power of evaporating fluids in producing cold, is nearly according to the degree of volatility in each. If to this we join the conſideration, that the cold is made greater by whatever haſtens the evaporation, and particularly, that the ſinking of the thermometer is greater as the air in which the experiment is made is warmer, if dry at the ſame time; I think we may now conclude, that *the cold produced is the effect of evaporation.*

I did not think it neceſſary to diverſify my experiments further by examining a great many fluids, which are manifeſtly of a like nature with theſe above mentioned. I preſume pretty confidently, that the ſeveral ſpirituous, watery, and oily fluids, a-kin to theſe already tried, will be found all of them to have ſimilar effects. And, conſidering how many fluids theſe claſſes comprehend, and that, in theſe already tried, the cold produced ſeems to depend more on the volatility of the aggregate than on the nature of the mixt; I was ready to conclude, that all fluids whatever would, in evaporating, produce cold. But I have found a ſeeming exception. When the ball of the thermometer is moiſtened with any of the foſſil acids, a conſiderable degree of heat is produced. It is however to be doubted, if this affords an exception. We know that theſe acids attract water from the air; and alſo that theſe acids, mixed with water, always produce heat: It may therefore be ſuppoſed, that the heat produced by moiſtening the ball of the thermometer with theſe acids, is to be imputed rather to their mixing with the water of the air, than to their evaporation ſingly. This perhaps cannot be poſitively determined till the evaporation of theſe acids, in a very perfect vacuum, is examined; which I have not yet had an opportunity of doing. In the mean time, I have made an experiment which I think is to the purpoſe. To one part of ſtrong acid of vitriol, I added two parts of ſpring-water. When this mixture, which produces a great degree of heat, was returned to the temperature of the air, I uſed it for moiſtening the ball of the thermometer, and found it produced a ſenſible degree of cold, and ſeemingly a greater than water alone would have done. I need not here obſerve, that the mixture I uſed was ſtill a very acid liquor, only ſo much ſaturated with water, that it would not now attract any from the air. Whether it would not have had the ſame effect, tho' leſs diluted, I have not had time to examine. The experiment, as it ſtands, tends to prove, that the heat produced by acids, applied to the ball of the thermometer, is owing to the mixture of theſe with the water of the air; and therefore, it is ſtill very probable, that all fluids, which do not immediately affect the mixture of the air, will, in evaporating, produce cold.

When I had proceeded thus far, I began to conſider, whether the cold produced in the above experiments might not be the effect of the mixture of the ſeveral fluids with the air; and that therefore, to a liſt of cooling mixtures and ſolutions which I was then making up, I ſhould now add the ſeveral ſolutions made by the air. By one who ſuppoſes the evaporation of fluids to depend upon the action of the air as a menſtruum, this would be readily admitted; but, as I knew that fluids evaporate *in vacuo* as well as in the air, I reſolved to ſuſpend my opinion, till I ſhould repeat my experiments in an exhauſted receiver.

In proſecuting theſe, a number of new, and, to me, curious *phænomena* have preſented themſelves; ſo many, that I find the experiments muſt be often repeated, and much diverſified, before I can give the ſociety a proper account of them. In the mean time, I ſhall give you the following facts already ſufficiently verified.

A thermometer hung in the receiver of an air pump, ſinks always two or three degrees upon the air's being exhauſted. After a little time, the thermometer *in vacuo* returns to the temperature of the air in the chamber, and, upon letting air again into the receiver, the thermometer always riſes two or three degrees above the temperature of the external air.

When a veſſel containing ſpirit of wine, with a thermometer immerſed in it, is ſet under the receiver of an air-pump; upon exhauſting the air, the mercury in the thermometer ſinks ſeveral degrees. It becomes more eſpecially remarkable, when the air in any plenty iſſues out of the ſpirit of wine. As the ſpirit continues long to give out air, ſo it is long before the thermometer immerſed in it returns to the temperature of the external air. But when after ſome time it appears ſtationary, if it is then drawn up out of the ſpirits, and ſuſpended in the vacuum, the mercury ſinks very quickly eight or nine degrees; a good deal farther than it would have done in the ſame circumſtances in the air. In the ſame manner as in the air, the thermometer *in vacuo* may be made to ſink lower by repeated dippings into the ſpirit of wine: But here theſe repeated dippings have not ſo remarkable an effect as in the air; becauſe the dipping cannot be ſo quickly performed, and the thermometer is therefore more affected by the warmth of the ſpirit. It is ſometimes alſo affected by a drop of the ſpirit which the ball takes up along with it, and which, as I ſhould have obſerved above, ought always to be taken away in the experiments made in the air. This experiment with ſpirit of wine was often enough repeated, to ſhew clearly, that the evaporation of the ſpirit *in vacuo* produces a greater degree of cold than the evaporation of the ſame in the air.

Satisfied of this, I have tried alſo ſome other fluids, as the quick-lime ſpirit of ſal ammoniac, and the two kinds of æther. Veſſels containing theſe with a thermometer immerſed in the liquor, but ſeparately and at different times, were ſet under the receiver. With regard to the different fluids, the *phænomena* were much the ſame. Upon exhauſting the receiver, the fluid gave out a great quantity of elaſtic air; and, while this happened, the mercury in the immerſed thermometer ſunk very faſt, and to a great length. In our trials, it generally ſunk below the ſcale applied, ſo that we could not meaſure exactly how far. In one experiment, before exhauſting the receiver, the thermometer had ſtood at 50 degrees, and, after exhauſting, we could judge very certainly that it ſunk below 20. In another experiment made with the nitrous æther, when the heat of the air was about 53 degrees, we ſet the veſſel containing the æther in another a little larger, containing water. Upon exhauſting the receiver, and the veſſels remaining for a few minutes *in vacuo*, we found the moſt part of the water frozen, and the veſſel containing the æther ſurrounded with a thick and firm cruſt of ice.

SUCH a means of producing cold, and to ſo great a degree, has not, ſo far as I know, been obſerved before, and it ſeems to deſerve being further examined by experiments. Till that is done, I do not chuſe to give any account of ſome other remarkable *phænomena* that have occurred in the above experiments, nor to enter into the ſeveral ſpeculations that the ſubject ſeems to ſuggeſt.

SINCE writing the above, I have had occaſion to obſerve, that Mr Richman, of the Academy of Peterſburg, has taken notice of the effect of evaporating fluids in producing cold; but he does not impute it to the evaporation alone. His very exact account of the *phænomena*, and his theory with regard to them, may be ſeen in *Nov. Comment. Acad. Petropolitanae, ad ann.* 1747 *&* 1748, page 284.

FINIS.

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